

TEN YEARS OF NEUTRON PHYSICS  
WITH FELIX BLOCH  
AT STANFORD 1938-1949

*by Hans H. Staub*

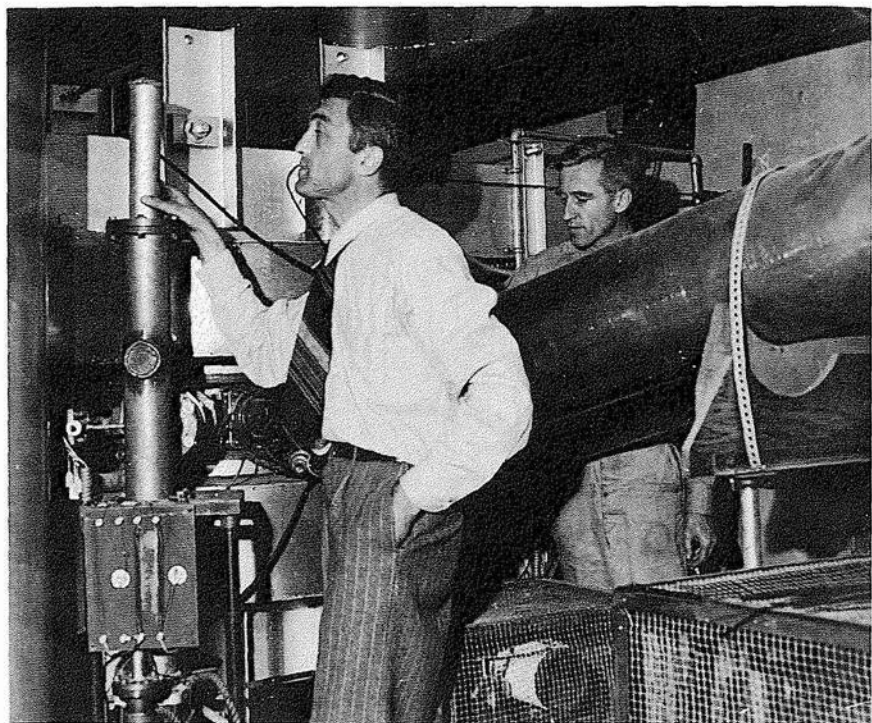
ABSTRACT

Work on the magnetic properties of the neutron occupied Felix Bloch and his colleagues during the pre-World War II years at Stanford. This paper gives a personal view of that work and describes the construction and use of the 27-3/4 inch cyclotron at Stanford by Bloch, Bradbury, Staub, and Stephens.

I met Felix Bloch for the first time when, as an assistant of W. Pauli, he had to correct and grade the weekly homework papers of the physics and mathematics students taking Pauli's lectures on classical theoretical physics. I do not think that Felix remembers my papers. They were certainly not brilliant but fortunately also not so catastrophically bad as to be easily remembered. While he was at Leipzig with Heisenberg and from 1933 on at Stanford, he quite often came to Zürich, and since only about two dozen physicists were at the ETH and the University of Zürich, we often met in colloquia, in seminars, and informally at some of the physics professors' homes. In 1937 I received an international student fellowship and a stipend from the ETH in Zürich to work at C. C. Lauritsen's Kellogg Radiation Laboratory at Cal Tech. On our way to Pasadena, by boat and train of course, my wife and I stayed for a few days in New York, where we did all the usual sight-seeing. One evening, while we were wandering in the enormous crowd on Times Square, my wife suddenly exclaimed, "Wasn't this Felix Bloch who just passed us?" Sure enough, he too had recognized us and turned around. He was on his way from California to Zürich for a visit to his parents and had taken an evening stroll with Melba Phillips. He

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FELIX BLOCH, standing at the dee support end of the Stanford cyclotron in 1946. The author (H. Staub) is in the background.

was greatly surprised that we took the same stroll in the opposite direction. The four of us had a merry evening. Felix took us greenhorns up to the sky-room of the Rockefeller Center. He also kindly took the key of my father's house, which by mistake I had taken along, back to my parents.

More than half a year later, when I started worrying over future employment, either in the USA or in Switzerland, I got a letter from Felix asking whether I would be interested in a position as instructor of physics at Stanford. He wanted very strongly to have the position filled with an experimentalist in nuclear physics, since together with Norris Bradbury and Howard Tatel he had just started the attempt to measure the magnetic moment of the neutron. I certainly was very much interested and with my wife and our first baby we drove up to Stanford, where I had to deliver the usual trial talk in the journal club. Apparently it was successful, and in September 1938 we moved to Stanford, where an unforgettable ten years of collaboration with Felix began.

Our first project was to obtain a sufficiently powerful source of neutrons. It was quite clear that the first pathetic experiments using Ra-Be sources for neutrons could not succeed for intensity reasons, although the temporal constancy of intensity was a great advantage. Keeping in mind that even a few hundred dollars were quite hard to pry loose from the department budget, we decided to build a d-d accelerator to be operated on the 170 KV voltage source that had been in existence at Stanford for some time and that was used for the famous x ray work of Webster, Kirkpatrick, and Hansen. For the construction of the accelerator tube I had quite a bit of experience from Zürich and Pasadena. The machine, practically all home made, was completed in the summer of 1939, but it was never used for work on the magnetic moment of the neutron. While the machine was still under construction, Felix got quite unexpectedly the opportunity to do the first successful magnetic resonance experiment with Luis Alvarez at the Berkeley Radiation Laboratory's 37" cyclotron. After completion, however, the Stanford machine was a suitable source of energetic neutrons for work on resonance scattering of neutrons on helium, which William Stephens and I had started at Pasadena and which was completed at Stanford together with Howard Tatel.<sup>1</sup> In this work Felix too participated by working out the theory of this particular type of resonance scattering.<sup>2</sup> Thus we were able to compare the measured maximum cross-section with the theory. Our measurements showed some evidence of a splitting of the  $P\ 3/2$  and  $P\ 1/2$  levels of a few hundreds KeV, but the confidence level was quite low. Indeed, it turned out later that the splitting was much larger. Our measurement of the cross-section, however, and this is the important point, was on an absolute scale (via comparison with the n-d and n-p cross-sections) and allowed therefore a comparison with Bloch's theoretical results without any open normalizing factor. The magnitude of the cross-section of the levels decreases, of course, rapidly with increasing splitting. Consequently the observed maximum cross-section of 0.7 barns showed that the splitting was at least somewhat larger than the width.

The work on the neutron magnetic moment with Alvarez had convinced Bloch that in order to do a more accurate experiment, it was necessary to study first several related problems, such as the polarization of neutrons by magnetic scattering and also the problem of the direct comparison with the proton magnetic moment. For all this work we simply needed a still more intense slow neutron source. Felix's mind was now dead set on getting a cyclotron for accelerating deuterons to some MeV with currents of some tens of microamps on a beryllium target. Such a device would be equivalent to a source of Ra-Be of some kilograms. I too shared his desire, but I was appalled to think of the financial consequences. We started, together with Norris Bradbury, and later W. E. Stephens, to do some serious engineering considerations with a few firm principles in mind: Let us not go

into any fancy development work; from Stanford to Berkeley it is only 30 miles to get competent advice; let us concentrate on a modest machine and avoid unnecessary changes in order to lower the cost so that the project can be realized with a few thousand dollars.

From then on things moved very quickly. Felix proved himself to be not only an outstanding theoretician of magnetism, but also a very able constructor of a large magnet. Moreover, he was a wizard in raising the four thousand dollars needed for the machine. Let me quote a footnote in the first of the many papers that resulted from the work with this machine<sup>1</sup>: "The construction of this 27-3/4" cyclotron by Bloch, Bradbury, Staub, and Stephens was made possible by grants from the Rockefeller Foundation, the Research Corporation and from various private sources. It was completed in the autumn of 1941, giving under normal operation a beam of about 10  $\mu$ amp of 2.5 MeV deuterons." We intended to publish a short account on the peculiar features of this machine, but like many other projects, our intention fell victim to heavy war-time activities. It seems therefore fitting to use this occasion for an account.

The magnet was built from stacks of 1-inch mild steel plates, machined only in places where it was necessary for stability and continuity of the flux. For economic reasons we decided to use forced air cooling, although this resulted in a relatively small power capacity and a relatively weak field of about 12 KG. By minimizing the costs of the magnet, we found a curious relation for the cost of the copper and iron. At the prevailing price level it turned out that these two amounts should be about equal. The magnet was energized by a 10 KW AC-DC converter generator, which the University owned as an emergency standby. Felix succeeded in extracting the machine from the office of buildings and grounds, provided it would be available to them at any time. To meet this condition we installed an immense antiquated double pole-double throw switch, but it was never used.

A particularly improbable incident just after completion of the machine remains unforgettable to me. It was on the last day of the spring quarter, the machine had been assembled to the last bolt, and we were ready to start testing and shimming the magnet. We felt that we had done a lot of hard work and deserved a few days of vacation, hiking in the Sierras. We still had some time left that afternoon, and so we picked, more or less at random, two equal circular iron plates from a stack that had been prepared for systematic shimming. To our utter surprise we got a beam of several microamps. Happily we shut down and went on our vacation. Upon returning a few days later we started a systematic shimming procedure. The two haphazardly chosen plates were taken out and replaced pairwise by slightly different ones. But whatever we did, even inserting the two former plates, we could not get even an indication of a beam. After two days of trying, we sat crestfallen around the machine until somebody had the bright idea to

have a good look at the two original plates. Sure enough, the two differed in thickness by less than  $1/64$  of an inch, and because of a slight mechanical asymmetry of the magnet, the thicker one had to be on the bottom. But this was accidentally just the case in the first try, and it was not even the sole incredible accident. It also turned out later that their size was exactly optimal.

The 10 KW, 10 Mc oscillator was of the conventional single tube tuned plate, tuned grid type, the tube being a demountable water cooled triode attached to a small oil diffusion pump. It had been built by Litton for a charity prize. The grid circuit consisted of a 30-cm diameter coaxial line of 10 m length. The low voltage arc ion source, which initially gave a deflected deuteron beam of about 10  $\mu$ amps, was later slightly modified, thereby increasing the current to 40  $\mu$ amps.

Concurrently with the construction of the cyclotron, we had to build all the clumsy electronic and neutron counters, which also required much room and power, by ourselves. The idea of having a technician or a machinist working full-time on our experiments never even entered our minds. Felix was a marvelous collaborator. Within a short time he became quite an expert in electronics and enjoyed these simple techniques. When a circuit he had built with his own hands worked just the way it was expected to, he was as pleased as a child with a toy. One incident was quite typical of his way of thinking in simple models and with uncomplicated conclusions. We had a problem concerning the shape of the undershoot from an RC coupled pulse amplifier. I told Felix that whatever magnitude R and C had, the area of undershoot would be the same as that of the upshoot. For an instant he seemed puzzled and asked how one could make such a general statement. When I replied that the output signal could not contain a DC component, he was delighted by the simplicity of this explanation.

It was also typical of Bloch that after the cyclotron was successfully operating, he would not attack the main experiment, the precision determination of the neutron magnetic moment, without thorough preparation. In order to do a thorough experiment, we had to get not only an intense slow neutron beam but even more important, a highly polarized one. Bloch himself had shown long before<sup>4</sup> that by magnetic scattering in ferromagnetic substances a beam of neutrons passing through magnetized iron would be appreciably polarized. But it was not understood why in a ferromagnet the deviation from complete saturation had to be so small. In fact, Bloch and Alvarez in their work had to use rather large electromagnets to magnetize the soft iron polarizer and analyzer plates in order to get a 6% single transmission effect from a 4-cm Swedish iron plate. Just about the time we started in 1941, Halpern and Holstein<sup>5</sup> showed that the high degree of saturation in a polycrystalline medium was necessary to avoid depolarization of the neutrons in the boundary zones of the microcrystals. The first

work with our machine by Bloch, Hamermesh, and myself<sup>3</sup> was a thorough study of the so-called single transmission effect, which gave a surprisingly good verification of the Halpern-Holstein theory and reasonable and quite accurate values for the magnetic scattering cross section and the size of the microcrystals. However, the result for the magnetic scattering cross section was found to be considerably larger than the value calculated by Hamermesh.<sup>6</sup> Since the first experimental data were taken with neutrons emerging from a block of paraffin of rather complex form, it was felt that single transmission measurements should be conducted with monochromatic thermal neutrons. For this purpose the cyclotron was pulsed and a time-of-flight arrangement, which shortly afterward came in very handy, was built by E. Fryer.<sup>7</sup> His results, however, were still higher than the theoretical value by about the same factor as before.

Any further work on the magnetic properties of the neutron came to an abrupt end in midsummer of 1942, when Felix and I, with the collaboration of M. Hamermesh and D. B. Nicodemus and others, took over a contract with the Manhattan District project prior to the formation of the Los Alamos Laboratory. Our task was the determination of the spectral distribution of the neutrons from fission induced by thermal neutrons in U 235. At that time this was quite a formidable experiment, since no enriched uranium was available. A direct spectral distribution measurement was out of the question, since even the most sophisticated moderator arrangement would still bring a large number of fast neutrons from the source into the detector and drown the few fission neutrons of about the same energy from a fission source of a few kg of natural uranium metal. The difficulties were overcome by two important experimental novelties. The first gave a purely thermal atmosphere of fission-inducing neutrons by capturing a burst of fast neutrons from the cyclotron in a cubic cavity of about 1 m<sup>3</sup> whose walls consisted of highly purified reactor graphite, which had just become available. The thermal neutrons had a lifetime of about 2 millise., so that after about one millise. none of the fast neutrons but practically all of the slow ones were present. The second trick concerned the recording sensitivity of the hydrogen recoil pulse ionization chamber. The primary fast neutron pulse put an enormous charge on the collecting electrode and the grid of the first tube connected to it, and would thereby paralyze the counter arrangement with its relatively large time constant for a considerable time. The difficulty was overcome by compensating this charge through a time modulated negative feedback arrangement. The result we obtained was quite good.<sup>8</sup> It differed from later precise measurements mainly by a 30% lower intensity of fast neutrons around 2 to 3 MeV. As we had suspected, this difference was due to an appreciable inelastic scattering of the fission neutrons in the large mass of uranium and mainly in the very thick walls of the ionization chamber container, whose dimensions had to conform with the California State safety regulations.



By the end of June 1943 we had completed this work and moved with some of the equipment to Los Alamos. A few months later Felix joined the radiation laboratory at Cambridge, Massachusetts, and our collaboration was interrupted for more than two years.

In September 1945, I returned for a few days' visit to Stanford. Felix had already been back for some time and had started peaceful work with great enthusiasm. I remember very well how, on a sunny afternoon in his garden, he told me that he believed he had found the really decisive method for comparing the magnetic moments of the neutron and proton. In his familiar and simple way, using purely classical ideas and models, he explained his experiment, and I heard for the first time about nuclear induction. He urged me to return immediately to Stanford to participate in this work, but much as I regretted it (and I still do), I had to return to Los Alamos until February 1946 for a lot of clean-up work. When I finally returned, Felix, Bill Hansen, and Martin Packard had just completed successfully the first nuclear induction experiments, using the same old 3-inch lecture demonstration magnet we had used for the neutron polarization experiments, whenever it was not needed for lecture demonstrations.

Fortunately the times of extreme poverty in pure physics were over by then, and although Felix and I still practiced quite a bit of sporty austerity, ONR and AEC grants gave one the comfortable feeling that the procurement of a modest magnet to provide a 10 KG field over some thousand c.c.'s no longer meant an uncertain and dangerous financial venture.

Thus, in a carefree atmosphere, our reassembled group of Bloch, Nicodemus, and myself started the precision measurement of the neutron magnetic moment, carefully preparing and checking each step without hurry. First of all, we repeated the pre-war experiments on neutron polarization in magnetized iron, extending the measurements to magnetizing fields of 10,000 Oersted. We were rather pleased to find that the saturation polarization was considerably larger than the previous value obtained from a large extrapolation. For a 3/4-inch thick iron plate the saturation value of the single transmission effect was almost 7%, corresponding to a polarization of the neutrons of 35%. This increase meant that the observable resonance would give a comfortable transmission effect with a maximum of about 15%.

Proton nuclear induction was used not only for the actual measurement of the magnetic moment ratio, but for the first time also for stabilization of the magnetic field.

In a little less than two years, between 1946 and 1948, the work was completed in all details.<sup>9</sup> The accuracy of the result was not improved by an order of magnitude until more than 25 years later. During the last two years of my activity at Stanford, Felix and I each directed separate projects in the field of nuclear magnetic moments, but we still cooperated a great deal, discussed our problems, and helped each other. With J. Fleeman and D. B.

Nicodemus, I did a final series of thorough experiments on neutron polarization, which cleared away the few remaining problems on polarization by magnetic scattering.<sup>10</sup>

With E. Rogers,<sup>11</sup> we did what we called an amusing but perhaps unnecessary experiment. We determined the signs of proton and neutron magnetic moments directly by using a true rotating field instead of an oscillating one. I remember vividly how pleased Felix and also I. I. Rabi were, that the transition experiment was done for once with a true rotating field. At that time, Bloch had just started with C. Jeffries the measurement of the value of the proton magnetic moment in terms of the nuclear magneton.<sup>12</sup> They used the ingenious method of the anticyclotron resonance, and I was happy that particularly during Felix's absence in Europe, I could give a hand in the instrumentation of that beautiful experiment.

Then, in September 1949, our collaboration of ten years came to an end, when I accepted the position of the chair of experimental physics at the University of Zürich.

I have often been asked why I felt that doing physics with Felix led to such a particularly agreeable and fruitful partnership, since after all, theoreticians and experimentalists also collaborate successfully in many other places. But I am convinced that the reason for this particularly productive collaboration lies in the fact that Felix's role was much more than that of a pure theoretician, who advises the experimenters, tells them what to do but not how, and possibly participates in the evaluation of data. Felix is simply just a physicist, recognizing the importance of both theory and experiment and acting correspondingly by participating in the combined activity. But likewise—and this again is a very positive asset—he expects justly that his collaborators understand the theory pertaining to their experiments quite thoroughly. This gift of looking at physics as a unity has unfortunately nowadays become rather rare.

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